ROYAL ASTRONOMICAL SOCIETY.

VOL. XII. *March* 12, 1852. No. 5.

Capt. W. H. SMYTH, R.N., Vice-President, in the Chair.

Isambard Kingdom Brunel, Esq., Duke Street, Westminster,
Edward Vogel, Esq., Assistant at Mr. Bishop's Observatory, South Villa, Regent's Park,

were balloted for and duly elected Fellows of the Society.

Discovery of a new Planet by Dr. A. de Gasparis.

"I have the honour to inform you that on the evening of March 17 I discovered a new planet. It resembles a star of 10.11 magnitude, and is at present near *Regulus*, with which I have compared it."

Apparent Positions.

1852.	Naples M.T.	R.A.	Decl.	No. of Obs.
	h m s	h m s	° ′ ″	
Mar. 17	9 52 32·5	9 57 56·7	+12 51 20	2
19	8 20 18·6	9 56 54·2	+12 58 19	7
20	9 25 27·1	9 56 21·2	+13 1 41	2

Note on the Discovery of the New Planet of De Gasparis.
By Mr. J. R. Hind, For. Sec. R. Ast. Soc.

On the 3d of April, letters arrived in this country from Professor De Gasparis, of Naples, announcing his discovery of a new planet on the evening of the 17th of March. It is described as resembling a star of the 10.11th magnitude.

The object of the present remarks is to show that the existence of this planet had been detected at Mr. Bishop's Observatory on the 18th of March, the night after the actual discovery at Naples.

On the 29th of January, during a final revision of the ecliptical chart for Hour x, prior to placing it in the engraver's hands, I entered an object shining as a star of the eleventh magnitude in R.A. $10^h\,32^m\,40^s$, Decl. $+8°\,59'$ (as nearly as the map will give it): some other small stars were entered the same evening, and the

chart was believed to be complete. Every night for the week following proved cloudy, and in order to cause as little delay as possible in the publication of the map, the working copy was taken to the engraver, and the re-examination of the few stars marked upon it towards the end of January was deferred until a proof arrived. Considerable delay occurred, and we did not receive a perfect copy before the 18th of March. The evening being very fine, I compared the whole chart with the heavens, and immediately missed the star of the eleventh magnitude observed on January 29. I was certain of its planetary character and commenced a search for it at once. Supposing it to have been a distant planet (which, from its proximity to the ecliptic and faintness, was by no means improbable), it could not have retrograded more than three or four minutes in right ascension; but, on carefully looking over the neighbourhood, there appeared to be no star of the eleventh magnitude unmarked in its proper place upon the chart. The search was, therefore, conducted on the supposition that the missing star belonged to the group of minor planets: in this case it had probably retrograded into the chart for 9^h R.A. at present in an unfinished state, and I forthwith commenced mapping in every star to the eleventh magnitude inclusive, confining my attention on the 18th chiefly to the small stars lying more than $1^\circ\frac{1}{2}$ from the ecliptic north and south. This appeared desirable, because a planet with an inclination of 8° or 10° might have passed over several degrees of latitude since the 29th of January. On March 20th I renewed the search, but found all the stars of March 18 in their places; I, therefore, inserted those situated nearer to the ecliptic, and amongst them, one placed on the map in R.A. $9^h\ 56^m\ 30^s$, Decl. $+\ 13^\circ\ 4'$, and noted as of the eleventh magnitude. During the ensuing fortnight the thick weather and moonlight effectually prevented us from carrying on the search, and at the time the announcement from Naples reached us we were merely waiting for a clear dark night to continue it. The observation by Professor de Gasparis on March 20 gives for the position of the planet a place agreeing so nearly with that of the star of eleventh magnitude inserted on our chart on the same night, that there can remain little doubt of my having again seen the planet, particularly as this star also is now missing.

In offering these remarks, I must disclaim on the part of Mr. Bishop and myself the most remote intention to interfere with the just claim of Professor de Gasparis to the merit of this discovery. A comparison of dates will prevent misapprehension on this point. The planet was actually detected as such at Naples on the 17th of March, while I was not aware of its existence until the following night. A week or two longer would no doubt have led to our recognising the planet again, had it escaped the admirable plan of examination of the heavens at present pursued by Professor de Gasparis.

Until the orbit of the new planet is calculated, we cannot, of course, decide whether it really occupied the position of the missing

star of January 29th; I am satisfied the object seen that night was a small planet, and the fact of my having actually mapped in the new one as a star of eleventh magnitude, on March 20, was entirely owing to this conviction.

The copy of the ecliptical chart for 10^h, in the possession of the Society, contains the star of the eleventh magnitude inserted on January 29, which there is reason to suppose to have been the new planet.

The following positions have been obtained with Mr. Bishop's equatoreal:—

1852.	Greenwich M.T. h m s	R.A. h m s	Decl. ° ′ ″
April 3	11 43 58	9 51 14·03	76 22 30·5
	12 4 25	14·00	33·7
4	8 38 18	9 51 3·94	76 21 20·6

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1852.	Green. M.T. h m s	R.A. h m s	Log $\frac{p}{P}$.	N.P.D. ° ′ ″	Log $\frac{q}{P}$.	Star of Comp.
April 7	8 35 26·1	9 50 33·75	−7·594	76 17 17·4	−9·8032	α Leonis
	8 52 23·2	33·52	−6·855	17·8	9·8027	—
	9 12 20·2	33·18	+7·428	15·6	9·8028	—
8	8 59 18·2	9 50 26·15	+7·031	76 16 10·9	9·8025	—
	9 17 15·6	25·93	7·632	9·3	9·8031	—
9	10 17 5·8	9 50 20·45	+8·186	76 15 5·7	9·8095	—
	10 47 1·0	19·80	8·305	4·4	9·8147	—
10	8 42 16·8	9 50 15·20	−6·730	76 14 14·8	9·8023	—
	9 0 14·0	14·85	+7·428	11·9	9·8024	—
11	8 17 21·6	9 50 11·57	−7·632	76 13 26·1	9·8023	—
	8 39 18·4	11·50	6·554	21·6	9·8022	—
12	8 33 19·1	9 50 8·79	−6·854	76 12 42·5	9·8021	—
	8 53 16·0	8·89	+7·456	40·2	9·8024	—
13	10 51 3·6	9 50 7·95	+8·369	76 12 3·1	9·8190	—
	11 11 0·8	7·80	8·416	0·1	9·8236	—
14	10 39 8·5	9 50 7·79	+8·343	76 11 35·4	9·8168	—
	10 59 5·2	7·69	8·397	35·3	9·8215	—
15	10 57 10·4	9 50 9·48	8·402	76 11 18·3	9·8220	—
	11 17 7·5	9·21	+8·447	17·8	−9·8272	—

The observations are corrected for refraction. The corrections to be applied for parallax in seconds of time and arc are represented by p and q. P is the equatoreal horizontal parallax.

The following is the assumed *mean* place of the Star of Comparison for 1852·0, derived from the Greenwich Observations for 1850.

	R.A.	N.P.D.
	h m s	° ′ ″
α Leonis	10 0 29·14	77 18 40·40

The sky on the 9th was very hazy, and the observations of the planet were marked "exceedingly faint and not good."

HAMBURG. (M. C. Rümker.)

1852.	Hamburg M.T. h m s	App. R.A. h m s	Decl. ° ′ ″	No. Obs.
April 11	8 58 14·6	9 50 11·84	+13 46 47·6	6
14	9 2	50 8·35	48 19·6	2
15	8 47	50 9·77	48 37·5	1
16	8 46 31·2	9 50 11·51	+13 49 1·2	6

Apparent Places of the compared Stars according to the Hamburg observations with the Meridian Circle.

	R.A. h m s	Decl. ° ′ ″
April 4	9 49 11·35	+13 51 30·0
16	9 49 11·34	+13 51 29·8

Elements.

Computed from the Naples Observation of March 19 and the South Villa Observations of April 3 and 12, by M. Edward Vogel, F.R.A.S., of the South Villa Observatory.

M	77 38 43·33	April 0·1852, 0h Berlin M.T.
π	33 36 43·23	M. Eq. 1852, Jan. 1.
Ω	151 11 36·52	
i	3 30 14·67	
φ	24 39 56·22	
Log *a*	0·4865700	
μ	660″·9250	
dl	= −0″·10	Computed − Observed.
db	= 0″·00	

By M. George Rümker.

From the *Naples* observations of March 19 and 29, and the *South Villa* observations of April 4.

M	= 280 25 21·5	1852, April 1·0, Berlin T.
π	253 56 20·8	Mean Eqx,
Ω	150 14 11·7	1852, Jan. 0·0.
i	2 43 8·1	
φ	7 42 56·3	
Log *a*	0·4965374	
Log μ	2·8052005	

These elements represent the middle observation exactly, but they must be considered merely as approximate.

Ephemeris. By M. E. Vogel.

For Berlin Mean Noon.

1852.	R.A. h m s	Decl. ° ′	Log Δ.
April 20	9 50 28	+13 49·3	0·45354
21	50 35	49·0	
22	50 43	48·6	
23	50 54	48·1	
24	51 5	47·6	·46376
25	51 17	46·9	
26	51 30	46·1	
27	51 44	45·2	
28	52 0	44·3	·47393
29	52 16	43·3	
30	52 33	42·2	
May 1	52 51	41·0	
2	53 10	39·7	·48402
3	53 30	38·3	
4	53 51	36·8	
5	54 13	35·2	
6	54 36	33·5	·49399
7	55 0	31·7	
8	55 25	29·9	
9	55 50	27·9	
10	56 16	25·9	·50381
11	56 43	23·8	
12	57 12	21·6	
13	57 41	19·3	
14	58 11	16·9	·51346
15	58 42	14·5	
16	59 13	12·0	
17	9 59 46	9·4	
18	10 0 19	6·7	·52294
19	0 53	4·0	
20	10 1 28	+13 1·2	0·52761

M. George Rümker has also been kind enough to forward an ephemeris calculated upon his elements, which, however, arrived after M. Vogel's elements were in type. The following quantities must be applied to M. Vogel's ephemeris to produce M. Rümker's.

	R.A. m s	Decl. ′
April 20	+0 16	−1·1
30	+0 47	−3·4
May 10	+1 32	−7·3

FLORA.

DURHAM. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1852.	Green. M.T.	R.A.	Exc. of Ephem.	N.P.D.	Exc. of Ephem.	No. of Comps. in R.A.	N.P.D.	Set.
	h m s	h m s	s	° ′ ″	s			
Mar. 2	12 50 19·9	12 49 50·72		85 50 17·2		24	8	1
3	12 6 35·7	49 11·21		85 43 7·2		18	6	2
9	12 28 14·6	44 37·28	− 9·77	84 57 23·5	−57·4	18	6	3
24	14 38 24·7	12 30 38·68	−10·06	83 3 3·9	−53·8	18	6	4

Corrected for refraction and parallax, and compared with the ephemeris circulated by the superintendent of the *Nautical Almanac*.

Assumed *Mean* Places of Stars of Comparison, 1852·0.

Name.	R.A.	N.P.D.	Set.
	h m s	° ′ ″	
B.A.C. 4340	12 48 9·16	85 47 50·2	1, 2
Weisse, xii^h, 787	46 8·98	84 57 19·8	3
— — 536	12 32 12·23	83 4 8·5	4

Set 1. Good observation. *Flora* about 9½ magnitude.
— 4. Sky very changeable. Measures obtained with difficulty.

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1852.	Green. M.T.	R.A.	N.P.D.	Comp^d—Obs^d		Stars of Comp.	
	h m s	h m s	° ′ ″	s	″		
April 8	11 13 54·5	12 16 24·25	81 34 27·5	−9·80	−51·4	B.A.C. 4072	
	11 39 49·5	23·03	23·3	9·54	52·3	—	—
10	10 15 27·2	12 14 41·99	81 25 49·8	−9·23	−52·1	—	—
	10 45 21·0	40·94	46·4	9·24	54·0	—	—
11	10 4 43·0	12 13 50·94	81 21 41·5	−8·72	−50·5	—	—
	10 29 38·6	50·25	35·2	8·91	48·5	—	—

The observations are corrected for refraction and parallax. The computed places are interpolated from the ephemeris circulated by the Superintendent of the *Nautical Almanac*.

The assumed *mean* place of the Star of Comparison for 1852·0, derived from the Greenwich Observations, is as follows :—

	R.A.	N.P.D.
	h m s	° ′ ″
B.A.C. 4072	11 57 40·23	80 26 41·55

EUNOMIA.

HAMBURG. (M. Rümker.)

1851.	Hamburg M.T.	App. R.A.	App. Decl.
	h m s	h m s	° ′ ″
Oct. 18	6 58 4·2	282 10 41·7	−20 52 24·7
25	6 46 19·8	284 29 49·3	−20 24 45·7

Not corrected for parallax.

PARTHENOPE.

HAMBURG. (M. Rümker.)

1851.	Hamburg M.T. h m s	App. R.A. ° ′ ″	App. Decl. ° ′ ″
Oct. 17	9 44 35·7	29 51 42·1	+3 45 56·6
18	11 4 14·6	29 37 4·3	3 40 0·1
Nov. 2	8 3 22·7	26 14 45·8	2 29 44·4
Dec. 19	10 57 35·9	22 49 0·0	3 5 13·8
25	6 57 3·3	23 24 34·3	+3 35 40·6

VICTORIA.

DURHAM. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1852.	Green. M.T. h m s	R.A. h m s	Exc. of Ephem. s	N.P.D. ° ′ ″	Exc. of Ephem. ″	No. of Comps. in R.A.	No. of Comps. in N.P.D.	Set.
Jan. 12	11 46 5·1	7 46 40·04	−4·53	80 1 41·2	−10·8	21	7	10
17	9 49 8·8	41 28·83	4·13	79 54 37·3	14·0	21	7	11
	13 19 27·5	20·13	4·64	23·0	14·4	22	8	12
18	12 53 28·6	40 17·96	4·30	52 42·6	15·9	15	5	13
19	9 9 50·6	39 24·72	4·08	51 16·3	(20·9)	3	1	14
23	10 19 10·8	35 14·41	4·24	42 63·3	12·7	24	8	15
	12 30 56·7	8·52	3·93	51·7	13·0	12	4	16
24	11 41 25·9	34 10·02	3·92	40 43·0	11·1	15	5	17
	12 28 59·6	8·63	4·52	40·2	12·7	15	6	18
25	9 44 51·6	33 15·20	4·23	38 39·4	11·5	10	5	19
	12 12 32·5	9·24	4·40	26·5	12·6	24	8	20
26	10 15 30·6	32 14·59	4·21	36 16·2	9·9	12	6	21
28	8 51 27·6	30 21·68	4·26	31 39·5	13·0	20	7	22
30	8 54 46·2	28 28·13	3·80	26 38·2	13·3	16	8	23
31	9 46 21·7	27 31·26	−3·92	23 56·5	−12·3	23	8	24
Feb. 5	10 7 5·2	23 11·73	...	10 20·8	...	16	8	25
6	9 0 6·8	22 25·62	...	79 7 38·0	...	24	8	26
9	10 44 43·3	20 5·36	...	78 58 51·6	...	9	3	27
10	8 58 1·9	19 25·87	...	56 7·6	...	12	4	28
13	12 42 12·4	17 19·56	...	46 49·4	...	24	8	29
15	8 57 13·1	16 12·48	...	41 21·7	...	10	5	30
21	9 58 8·2	13 10·27	...	23 15·4	...	24	8	31
22	9 51 12·7	7 12 45·82	...	78 20 18·6	...	18	6	32

The parallaxes and computed places for January were taken from the ephemeris for the opposition circulated by the Superintendent of the *Nautical Almanac*, based on the elements of M. Villarceau; the parallaxes for February were deduced from the values of log Δ, given in the *Berliner Astron. Jahrbuch for* 1854.

Victoria was on all occasions this year below the 10th magnitude, estimated according to Bessel's scale.

Assumed *Mean* Places of Stars of Comparison, 1852·0.

Name.	R.A. h m s	N.P.D. ° ′ ″	Set.
Weisse, vii^{h}, 1469	7 49 22·25	80 7 21·1	10
— — 1219	40 5·82	79 55 12·4	11
— — 1195	39 27·61	80 0 49·1	12
— — 1135	37 32·35	79 47 10·2	13, 14, 15
— — 821	26 51·07	42 39·7	16, 17
— — 906	29 27·12	38 8·8	18, 19, 21
— — 826	27 4·32	33 42·4	20, 22
— — 898	29 10·68	22 13·3	23, 24
— — 799	25 56·02	79 6 56·2	25, 26
— — 720	23 30·85	78 57 46·0	27, 28
— — 473	15 49·29	48 41·4	29
— — 555	18 30·21	42 0·0	30
— — 302	7 10 2·90	78 18 47·2	31, 32

Sets 19, 21, 23, 25, and 30, were taken by the hearing-tube, in consequence of high winds.

— 12. The N.P.D. of Weisse, vii^{h}, 1195, here depends on 1174 and 1219.

— 14. The N.P.D. is worthless: it is the result of a single bisection under very unfavourable circumstances.

— 27, 28. Interrupted by haze.

— 29. Sky foggy, and full of snow. Planet badly seen.

— 31. Many small stars in the field, one following in 5^{s}; but no doubt about the identity of the planet.

Some Corrections of Weisse's Catalogue.

Weisse, vii^{h}, 761, appears to have originated in an error of 1 revolution = 34″·1 in declination: assuming this, its declination for 1825 will be + 10^{h} 56^{m} 17^{s}·5, and the star becomes identical with 760. There is no such double star as these would otherwise form.

— — 986. No such star exists in the heavens, nor can I find it in Bessel's Zones.

— — 1135, is wrongly reduced in R.A. Its R.A. for 1852·0 should be 7^{h} 36^{m} 3^{s}·49.

— — 1174, requires 2^{m} to be added to its R.A.; and the precessions must be looked to accordingly.

— — 1469. The seconds of R.A. should be 53^{s}, not 55^{s}.

Hamburg. Equatoreal. (M. C. Rümker.)

1852.	Hamburg M.T. h m s	App. R.A. h m s	App. Decl. ° ′ ″	Comp.
Jan. 21	9 52 33·3	114 20 5·6	+ 10 12 43·9	10
22	8 12 52·5	114 5 48·7	14 28·8	15
24	7 29 18·6	113 35 51·7	18 46·7	2
26	9 23 9·0	113 4 44·9	23 24·1	11
27	7 47 7·0	112 51 1·8	+ 10 25 41·2	1

Apparent Places of Stars compared with *Victoria.*

	App. R.A. h m s	App. Decl. ° ′ ″	Comp^{d}.
Jan. 21	7 37 32·859	10 12 45·3	Jan. 21 and 22
24	33 37·312	2 47·5	24 and 26
27	7 27 4·845	10 26 12·8	27

EGERIA.

Liverpool. Equatoreal. (Mr. Hartnup.)

1852.	Green. R.A. h m s	R.A. h m s	N.P.D. ° ′ ″	Comp^{n}—Obs^{n}. R.A. s	N.P.D. ′ ″	Star of Comp.
Mar. 18	10 46 10·0	11 50 49·01	65 19 1·3	−49·73	−5 15·0	B.A.C. 3990
	11 6 6·1	48·18	3·2	49·81	5 16·7	— —
20	10 20 10·6	11 48 38·85	65 20 7·3	49·49	5 10·5	— 4056
	10 35 7·6	38·11	8·9	49·42	5 11·5	— —
23	9 19 19·7	11 45 27·72	65 23 59·3	48·91	5 8·2	— 3964
	10 41 15·2	26·62	60·0	−48·73	−5 7·4	— —

The observed places are corrected for refraction and parallax. The computed places were deduced from the ephemeris circulated by the Superintendent of the *Nautical Almanac*.

The following are the assumed *Mean* Places of the Stars of Comparison for 1852·0:—

	R.A. h m s	Decl. ° ′ ″	Authorities.
B.A.C. 3990	11 40 20·80	68 57 30·25	Greenwich and Edinb. Observations.
— 4056	54 9·34	67 4 50·30	Edinburgh Observations.
— 3964	11 53 5·08	67 49 29·37	— —

Durham. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1852.	Green. M.T. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	No. of Comps. in R.A.	N.P.D.	Set.
Jan. 25	13 48 46·7	12 25 42·92	−8·387	68 58 51·3	−9·7722	24	8	1
	14 22 55·4	43·42	8·284	48·2	9·7602	18	6	2
28	12 33 21·0	25 54·01	8·514	44 70·8	9·7992	14	6	3
	13 12 18·6	53·81	8·446	53·8	9·7806	18	6	4
	14 41 31·3	53·99	8·154	32·7	9·7492	15	5	5
30	12 58 28·3	25 51·53	8·460	35 4·7	9·7819	15	7	6
31	12 21 43·9	25 47·25	8·515	30 8·9	9·7973	10	5	7
Feb. 3	12 15 51·1	25 22·82	8·506	14 26·9	9·7921	12	4	8
5	14 2 37·0	24 55·91	8·187	68 3 12·0	9·7432	18	6	9
6	13 39 30·4	24 40·35	8·268	67 57 46·0	9·7480	24	8	10
10	12 36 14·1	23 16·52	8·409	35 34·9	9·7621	24	8	11
	13 20 6·9	15·99	8·277	23·5	9·7445	24	8	12
13	13 21 38·5	21 51·26	8·221	67 18 24·3	9·7369	24	8	13
17	12 40 53·3	19 32·96	8·306	66 56 3·9	9·7405	23	8	14
18	12 29 31·7	18 53·54	8·330	50 43·0	9·7423	18	6	15
19	13 24 49·4	18 10·69	8·056	45 4·8	9·7222	24	8	16
25	11 36 23·8	13 31·95	8·391	14 41·7	9·7447	11	4	17
27	12 30 45·3	11 43·63	8·152	66 5 15·3	9·7183	22	7	18
Mar. 2	11 55 16·2	7 56·69	8·233	65 48 53·2	9·7217	17	6	19
3	11 16 9·7	6 58·56	8·356	45 19·3	9·7333	24	8	20
9	11 17 13·8	12 0 42·36	8·249	27 58·1	9·7175	12	6	21
12	12 0 21·0	11 57 24·30	−7·867	22 31·7	9·7006	18	6	22
20	12 4 24·3	48 34·14	+6·888	20 17·5	9·6971	18	6	23
24	11 17 6·1	44 19·40	−7·625	26 6·0	9·6995	18	6	24
27	10 47 36·9	11 41 15·37	−7·898	65 33 32·6	−9·7066	9	3	25

Corrected for refraction only. The log-factors for parallax are given, as the ephemeris in the Berlin *Jahrbuch* was not quite satisfactory.

Assumed *Mean* Places of Stars of Comparison, 1852·0.

Name.	R.A. h m s	N.P.D. ° ′ ″	Set.
Bessel's Zone, 362	12 23 52·56	68 57 26·6	1
— — 362	29 24·87	56 56·6	2
Anonymous, 9th mag.	26 3·00	44 55·0	3
Bessel's Zone, 467	20 39·15	49 31·0	4
— — –	30 52·32	43 8·9	5
— — –	31 13·12	27 51·0	6, 7
— — –	24 44·42	15 43·3	8
B.A.C. 4260 (Edin. Obs.)	31 45·50	68 7 21·0	9
Lalande, 23596	29 48·00	67 59 7·0	10
Bessel's Zone, 467	27 23·99	38 1·8	11
Anonymous, 8th mag.	24 9·80	36 14·0	12
Bessel's Zone, 467	22 55·47	13 52·2	13
— — –	17 3·98	67 0 30·7	14
Anonymous, 9½ mag.	17 17·67	66 48 32·4	15
Bessel's Zone, 412	14 56·77	42 52·4	16
B.A.C. 4141	11 50·50	66 8 32·0	17, 18
Bessel's Zone, 412	4 13·84	65 51 8·1	19, 20
— — –	12 0 30·09	28 26·5	21, 22
— — 353	11 45 14·32	23 44·4	23, 24
— — –	11 32 42·72	65 34 55·6	25

Set. 1. *Egeria* fully 10th magnitude.
Sets 5, 23, 24. Sky foggy, and planet dim.
— 6, 7, 9, 14, 15, 16. Windy. Boisterous. The hearing-tube used.
— 8, 17, 25. Measures interrupted by cloud.
On other nights circumstances were favourable.

ENCKE'S COMET.

CAMBRIDGE, U.S. Great Equatoreal. (Prof. W. C. Bond.)

1852.	Camb. M.T. h m s	R.A. Comet—✱ m s	No. of Obs.	Decl. Comet—✱ ′ ″	No. of Obs.	Star of Comp.
Jan. 13	6 57 28·46	+0 8·85	17	−2 2·91	6	*a*
17	7 2 16·71	+0 15·08	16	−3 32·96	10	*b*
19	7 24 35·76	−0 21·22	9	+3 32·53	6	*c*
20	6 54 5·46	−0 6·73	10	+3 35·38	6	*d*
23	7 20 35·57	−1 5·42	8	−3 38·27	5	*e*
Feb. 2	6 58 23·46	+0 35·04	12	+3 6·80	10	*f*
14	7 35 38·39	+1 34·51	8	+2 50·03	6	*g*

Stars of Comparison referred to the Mean Equinox of Jan. 1, 1852.

Star.	R.A. h m s	Decl. ° ′ ″	Mag.	Authority.
a	23 6 29·23	+4 11 34·56	8	Weisse, xxiii, 111
b	11 18·36	36 3·28	8.9	compared with B.A.C. 8127
c	14 31·28	41 39·93	10	— — — —
d	15 34·75	4 47 57·40	...	— — — —
e	20 44·02	5 15 39·58	8	Weisse, xxiii, 413
f	34 24·43	6 25 54·91	6.7	— — 710
g	23 54 49·65	+8 7 57·25	9.10	— — 1132

Hence the following places of the comet are deduced.

1852.	Cambridge M.T. h m s	R.A. Comet. h m s	Decl. Comet. ° ′ ″
Jan. 13	6 57 28·46	23 6 38·08	+4 9 31·65
17	7 2 16·71	11 33·44	32 30·32
19	7 24 35·76	14 10·06	45 12·45
20	6 54 5·46	15 28·02	4 51 32·78
23	7 20 35·57	19 38·60	5 12 1·31
Feb. 2	6 58 23·46	34 59·47	6 29 1·71
14	7 35 38·39	23 56 24·16	+8 10 47·28

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1852.	Green. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Star of Comp.
Jan. 28	6 24 57·0	23 26 39·82	84 13 9·1	*a*
	6 39 56·2	40·98	9·4	–
Feb. 6	6 57 9·3	23 41 24·98	82 58 72·8	*b*
	7 25 8·8	27·73	48·6	–
10	6 54 20·8	23 48 32·09	82 24 74·9	*c*
	7 27 30·4	34·10	63·0	–
	7 42 30·2	35·30	52·4	–
19	6 58 4·1	0 5 32·77	81 12 40·3	*d*
	7 13 3·7	33·80	34·9	–
	7 28 3·9	35·02	33·5	–
	7 43 4·1	36·69	26·5	–
28	7 17 8·2	0 22 6·42	80 39 19·1	*e*
	7 39 8·7	8·18	23·7	–
Mar. 2	7 1 34·7	0 26 20·86	80 53 28·3	*f*
	7 13 34·6	21·63	31·7	–
	7 25 34·7	22·47	38·0	–
	7 37 35·1	22·71	40·4	–
7	7 3 42·6	0 29 9·44	82 21 48·0	*g*
	7 21 42·6	9·15	68·2	–
9	7 7 23·7	0 27 40·22	83 32 50·1	–
	7 27 24·5	39·24	80·0	–

The observations are corrected for refraction and parallax; the log distance for parallax was deduced from the ephemeris circulated by the Superintendent of the *Nautical Almanac*.

The following are the assumed *Mean* Places of the Stars of Comparison for 1852·0.

		R.A. h m s	N.P.D. ° ′ ″	Authorities.
a	B.A.C. 8233	23 32 20·40	85 10 30·91	Greenwich Observations, 1849
b	— 8331	23 51 42·82	83 57 20·93	— — 1850
c	— 8354	23 54 56·22	82 20 12·00	Edinburgh Observations.
d	— 66	0 12 59·06	82 37 55·25	Greenwich 12-year Catalogue.
e	— 137	0 26 30·83	80 30 42·92	Edinburgh Observations.
f	— —	0 20 41·13	80 37 15·16	Santini's Catalogue of 1677 Stars.—*Mem. R.A.S.* vol. xii.
g	— 222	0 41 0·40	83 13 15·81	Greenwich Observations.

The light of the comet appeared to me much less diffused than it was in 1848–9. On favourable occasions, the appearance of the comet, with a low power, resembled that of a nebulous star, and the nucleus admitted of very accurate bisection. This was more particularly the case on and after the 19th of February.

DURHAM. Fraunhofer Equatoreal. (Mr. R. C. Carrington.)

1852.	Green. M.T. h m s	R.A. h m s	Exc. of Ephem. s	N.P.D. ° ′ ″	Exc. of Ephem. ″	No. of Comps. in R.A.	N.P.D.	Set.
Jan. 17	6 41 16·1	23 11 14·94	+ 0·63	85 28 9·0	+ 52·3	6	6	1
	7 32 11·5	17·95	0·31	10·6	37·9	6	6	2
Feb. 8	7 5 15·8	44 56·04	0·59	82 42 1·7	4·6	9	3	3
9	6 55 19·6	23 46 42·90	0·48	82 33 32·5	+ 1·4	24	8	4
18	7 3 31·9	0 3 37·45	1·45	81 19 43·5	— 17·1	18	6	5
19	7 21 34·8	5 34·46	2·07	81 12 33·6	24·4	10	5	6
21	7 16 27·8	9 25·78	2·49	80 59 37·9	36·0	24	8	7
22	7 7 40·5	11 19·92	3·00	53 54·5	35·9	24	8	8
23	7 11 52·4	13 13·78	3·81	48 55·4	43·5	18	6	9
24	7 12 57·0	15 6·83	3·65	44 39·7	45·9	18	6	10
28	7 27 31·8	22 7·32	7·15	39 10·3	83·2	14	5	11
Mar. 2	7 46 0·5	26 23·98	10·18	80 53 42·9	140·6	12	6	12
3	7 15 5·6	0 27 27·15	+ 12·75	81 3 23·8	— 156·7	12	4	13

Corrected for refraction and parallax, and compared with the ephemeris circulated by the Superintendent of the *Nautical Almanac*.

Assumed *Mean* Places of Stars of Comparison, 1852·0.

Name.	R.A. h m s	N.P.D. ° ′ ″	Set.
B.A.C. 8127 (Edin. Obs.)	23 12 48·00	85 25 29·7	1
Weisse, xxiii^h, 252	12 38·19	85 29 38·6	2
— — 983	23 48 3·90	82 36 2·0	3, 4
Anonymous, 9th mag.	0 1 38·20	81 12 45·0	5
Weisse, 0^h, 126	7 48·65	81 4 16·3	6, 7
— — 219	12 51·82	80 53 6·2	8
— — 280	16 19·99	53 54·4	9
— — 317	19 15·68	40 13·1	10
— — 340	20 41·40	80 37 9·9	11
— — 436	25 49·61	81 2 48·0	12
— — 514	0 30 27·76	80 59 5·9	13

Encke's comet was first seen at Durham on January 12, and again on January 16, but high winds on both nights prevented more than a rough estimation of its position. On the 17th it was a little brighter, but still very faint, and the measures were in consequence taken with the ring-micrometer. On and after February 8 the comet was considerably brighter, and exhibited a strong condensation of central light, which admitted of accurate observation by the wire-micrometer. On the earlier evenings the coma seemed to spread slightly towards the s. f. direction, but subsequently no elliptical form was perceived. The whole extent of the coma was below two minutes of arc in all cases.

Sets 3 and 4. W. xxiii, 983, is also B.A.C. 8315, but as the B.A.C. place depends on a single observation by Flamsteed, I have preferred Bessel's. The star does not occur in any other catalogue in our collection.

Set 5. Wind troublesome. The star depends on equatoreal comparisons only.

— 6. High wind, and a brilliant aurora. The hearing-tube used. The star's right ascension by catalogue has been increased 1^s, after comparison with two neighbours.

— 10. Attention distracted by the cathedral bells.

— 11. High wind and cloudy. The hearing-tube used.

— 12. Right ascension measures not good: the star was inconveniently situated.

— 13. Favourable. The comet was seen once after this, for ten minutes only, on March 9, but no observation could be procured from the want of a visible star within reach.

HAVERHILL. (Mr. W. W. Boreham.)

1852.	Green. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	No. of Obs.	Star.
Feb. 10	6 44 45	23 48 30·15	82 25 10·6	4	*a*
11	6 50 14	50 21·82	16 17·9	6	*a*
12	6 54 34	52 11·08	82 7 59·4	6	*b*
13	6 43 29	23 54 2·86	81 59 50·1	5	*b*
18	7 27 38	0 3 37·55	19 27·0	3	*c*
20	7 2 55	7 28·33	81 5 46·9	3	*c*
22	6 57 43	11 19·12	80 54 0·4	6	*d*
27	6 58 6	20 27·46	38 22·4	3	*e*
28	6 38 27	22 4·66		3	*e*
	6 41 6		39 10·9	2	*e*
March 2	6 51 26	26 21·01	80 53 29·9	6	*f*
4	6 52 16	28 19·43	81 16 19·0	4	*g*
5	6 56 6	28 56·08	33 46·6	3	*g*
6	6 53 50	29 13·03	81 55 1·2	4	*g*
7	6 47 38	0 29 9·42	82 23 8·8	5	*h*

Assumed *Mean* places of Stars of Comparison, 1852·0.

	Name.	R.A. h m s	N.P.D. ° ′ ″
a =	B.A.C. 8315	23 48 3·82	82 35 57·1
b =	— 8353	23 54 49·50	81 52 1·2
c =	Weisse, 0^h, 69	0 4 12·17	81 40 57·3
d =	— — 151	0 9 8·93	80 34 41·9
e =	— — 340	0 20 41·37	80 37 9·7
f =	B.A.C. 137	0 26 30·84	80 30 44·0
g =	— 177	0 33 33·65	81 27 9·5
h =	Rümker, *Ast. Nach.* No. 795	0 32 3·22	82 41 25·1

The observations are corrected for refraction, and for parallax by Lieut. Stratford's ephemeris.

The first five or six observations were all taken under unfavourable atmospheric circumstances.

March 2 was pretty good. Probable error E = R.A. $\pm 0^s.034$; $\Delta \pm 0''.85$. The last observation is somewhat doubtful.

HAMBURG. (M. C. Rümker.)*

1852.	Hamburg M.T. h m s	App. R.A. h m s	App. Decl. ° ′ ″	Comp.
Jan. 20	6 43 6·0	348 47 4·0	+4 50 2·0	14
22	6 43 53·2	349 28 17·6	5 3 20·0	8
26	6 41 32·3	350 54 9·6	5 32 20·6	21
27	6 37 1·8	351 16 41·1	+5 39 23·4	3

Not corrected for parallax.

Mean Places of Stars for Jan. 1, 1836, in the Orbit of the Comet of Encke, extracted from the part of M. C. Rümker's Catalogue now about to be published.

Mean R.A. h m s	Prec. s	Mean Decl. ° ′ ″	Prec. ″	No. of Obs.	Mag.
23 2 53·960	+3·044	+4 6 58·38	+19·42	1	9
4 56·227	·044	50 54·90	·47	1	9
8 47·049	·044	33 0·38	·54	1	9
10 25·289	·045	33 27·20	·57	1	
11 59·445	·046	29 14·40	·61	2	*b* Pisc.
13 42·457	·046	4 36 24·71	·63	3	8.9
14 39·520	·042	5 39 10·20	·65	1	
15 2·400	·048	4 20 34·00	·66	2	8
16 0·774	·045	5 8 48·90	·67	1	8
16 50·402	·051	4 1 18·67	·69	1	9
19 39·006	·045	5 28 44·23	·73	4	*ℓ* Pisc.
19 55·384	·047	10 24·57	·74	2	8
21 11·457	·047	12 2·71	·76	4	9
22 1·684	·047	5 31 22·30	·77	1	9
22 33·903	·044	6 16 24·10	·78	1	9.10
23 14·449	·046	5 50 44	·79	1	
23 52·556	·045	6 10 58·31	·79	2	7.8
25 19·297	·052	4 39 20·89	·81	1	
27 54·143	·049	5 57 16·66	·85	2	8
30 48·596	·052	17 37·78	·88	2	8
32 47·965	·053	21 24·6	·90	1	9.10
32 48·966	·053	5 21 15·54	·90	1	8
23 35 34·797	+3·052	+6 20 26·51	+19·93	3	Dup.

* The MSS. of M. Rümker's contributions to this number of the *Notices* has been unfortunately mislaid between composition and revision.

Mean R.A. h m s	Prec. s	Mean Decl. ° ′ ″	Prec. ″	No. of Obs.	Mag.
23 36 26·941	+3·053	+6 16 54·55	+19·94	5	7.8
37 53·764	·054	6 16 13·28	·95	2	9
39 49·916	·053	7 20 8·11	·96	4	7
41 17·846	·053	42 57·11	·98	2	8
42 54·733	·055	42 59·76	·99	1	
44 19·579	·056	48 29·15	+19·99	4	6
45 15·287	·057	7 35 30·70	+20·00	1	8.9
45 41·626	·056	8 18 54·43	·00	2	8.9
46 0·281	·056	8 14			9
47 14·778	·058	7 18 42·92	·01	1	7
50 21·064	·060	8 20 1·39	·02	2	9
50 25·855	·059	8 38 19·55	·02	2	8.9
54 0·134	·065	5 25 17·63	·04	1	8.9
54 0·685	·063	8 2 37·10	·04	1	6
54 1·792	·062	9 52 3·83	·04	1	9
54 5·136	·063	7 28 54·31	·04	2	8
54 6·911	·063	34 27·31	·04	3	6
54 40·180	·064	21 34·44	·04	1	8
54 40·530	·064	7 20 59·6	·04	1	
56 10·863	·066	6 21 7·0	·04	1	
23 56 55·378	+3·066	+7 52 35·52	+20·04	2	8

Mean Places for 1850 from the new Series, now in print.

Mean R.A. h m s	Prec. s	Mean Decl. ° ′ ″	Prec. ″	No. of Obs.	Mag.
0 6 1·415	+3·075	+6 6 19·27	+20·05	3	9
9 26·186	·078	7 4 17·15	·04	2	9
9 51·724	·078	24 25·47	·03	1	
10 10·051	·078	1 5·11	·04	1	8
12 11·142	·080	42 0·89	·03	2	
12 52·914	·080	7 21 24·43	·02	1	
14 16·850	·080	6 10 46·68	·02	1	9
17 8·816	·083	6 58 28·88	·00	1	9
17 24·269	·083	7 2 4·98	+20·00	1	
17 35·253	·085	8 14 10·50	+19·99	1	9
17 58·129	·083	6 51 41·55	·99	1	7
18 4·389	·087	9 35 26·57	·99	1	7
18 15·555	·087	8 31 51·03	·99	1	
19 8·105	·085	8 29 14·36	·98	1	8
19 10·356	·085	7 43 56·21	·98	2	8.9
19 50·412	·083	6 24 13·11	·98	2	9
20 34·848	·090	9 22 6·76	·97	1	7
21 7·163	·087	7 33 17·31	·97	2	8
0 21 46·706	+3·092	+9 33 23·97	+19·97	1	8

Mean R.A. h m s	Prec. s	Mean Decl. ° ′ ″	Prec. ″	No. of Obs.	Mag.
0 22 9·764	+3·098	+8 8 37·01	+19·96	2	
23 19·517	·090	28 37·97	·95	1	7
23 52·674	·091	8 19 58·48	·94	1	7
24 39·575	·086	6 7 34·71	·94	2	6.7
26 1·086	·089	6 51 42·77	·93	1	9
26 24·679	·096	9 28 41·72	·92	4	7
27 9·240	·097	9 28 45·51	·91	1	
27 22·702	·090	6 53 45·28	·90	1	
28 18·684	·089	19 45·80	·90	1	
31 57·036	·094	6 17 55·14	·86	1	8
33 45·261	·101	8 48 1·51	·84	1	
36 34·216	·097	6 57 2·00	·80	1	8.9
36 58·465	·096	6 47 49·11	·79	2	9
39 26·964	·100	7 19 31·94	·76	1	
40 19·592	·109	9 23 52·65	·74	2	9
40 29·444	·110	9 26 26·35	·74	2	9
40 54·261	·099	6 46 4·00	·74	2	6
42 43·090	·098	6 17 27·08	·71	7	9
42 45·141	·113	9 35 37·61	·71	1	9
44 18·391	·115	9 47 9·19	·68	1	
44 44·958	·107	7 50 56·28	·67	1	8.9
44 55·735	·102	6 53 6·92	·67	1	9
44 59·256	·112	8 59 20·80	·67	1	
45 52·336	·115	9 28 38·69	·65	1	
46 3·234	·101	6 27 37·86	·65	1	9
47 33·217	·100	6 2 19·90	·63	1	8.9
48 44·910	·113	8 24 57·59	·60	1	7.8
50 10·838	·105	6 47 56·51	·57	1	8.9
50 32·898	·102	6 1 58·18	·57	1	7.8
52 3·356	·101	5 40 22·90	·54	1	7
54 19·202	·110	7 7 47·80	·49	1	
55 9·564	·110	7 4 53·48	·48	4	5
56 0·134	·105	5 57 29·14	·46	1	8
56 48·706	·121	8 46 50·39	·45	1	8
0 58 52·917	+3·119	+8 3 45·03	+19·40	1	

SOLAR ECLIPSE *of July* 28, 1851.

Liverpool Observatory. Mr. Hartnup.

Latitude 53° 24′ 47″·7 N. Longitude $0^h\ 12^m\ 0^s{\cdot}05$ W.

"The observations were made with the $8\frac{1}{2}$-inch equatoreally-mounted achromatic refractor. The first impressions which I saw on the sun's disc, were made simultaneously by two lunar moun-

tains at $1^h\ 57^m\ 47^s{\cdot}3$ Greenwich mean time. The intermediate portions of the moon's border, between these mountains, did not come in contact with the sun's disc till about four seconds later. At the conclusion of the eclipse, the appearance resembled two waves running towards each other along the sun's limb and meeting at the last point of contact. At the instant of meeting the convexity of the sun's border became perfected, and this instant was seized as the termination of the eclipse. It took place at $4^h\ 11^m\ 13^s{\cdot}2$ Greenwich mean time. It was my intention to have observed the eclipse in accordance with the systematic directions drawn up and circulated by the Astronomer Royal, but the intervention of clouds prevented this being done to any great extent. The following are all the observations that could be obtained :—

Liverpool Sid. Time. h m s	Instrumental N.P.D. ° ′ ″	Phenomena observed.
9 41 28·7	70 39 34·95	Sun's N. L.
9 42 32·7	71 11 9·50	— S. L.
9 48 4·7	70 39 38·05	— N. L.
9 49 43·7	71 11 11·30	— S. L.
9 51 35·7	70 39 39·30	— N. L.
9 53 0·7	71 11 14·25	— S. L.
9 54 24·7	70 39 41·85	— N. L.
9 55 40·7	71 11 17·35	— S. L.
10 12 36·8	70 54 3·30	South Cusp.
10 13 32·8	44 52·70	North Cusp.
10 15 20·8	39 54·70	Sun's N. L.
10 16 33·8	56 9·80	South Cusp.
10 18 13·8	42 24·40	North Cusp.
10 19 4·8	39 59·60	Sun's N. L.
10 20 15·8	57 34·55	South Cusp.
10 21 19·8	41 39·85	North Cusp.
10 22 4·8	70 40 0·95	Sun's N. L.
12 10 15·2	71 2 34·20	South Cusp.
12 11 31·2	70 44 34·90	North Cusp.
12 15 33·2	0 50·35	South Cusp.
12 17 9·3	50 45·10	North Cusp.
12 18 59·3	40 32·50	Sun's N. L.
12 26 27·3	70 40 33·15	— N. L.
12 28 18·3	71 12 5·00	— S. L.
12 30 21·3	70 40 33·75	— N. L.
12 33 35·3	71 12 5·10	— S. L.

"I was assisted in the preceding observations by Mr. Towson, the scientific examiner of masters and mates of ships in the merchant service for the port of Liverpool. Mr. Towson took charge of the upper microscope, while I made the bisection, noted the time, and read the lower microscope.

"At the time of greatest obscuration, the sun was nearly obscured by a sheet of cirro-stratus cloud; the whole heavens presented a dull, sombre appearance, and two or three gentlemen present complained of chilliness, although the thermometer in the shade was as high as 68°, and only 1°·5 lower than at the commencement of the eclipse."

Whitehaven. By Mr. J. F. Miller.

The greatest phase occurred at $3^h\ 3^m$ Greenwich M.T., when about 0·86 of the sun's disc was covered. The moon's edge, on the whole, was well defined, though somewhat serrated towards the sun's lower cusp. The sun's surface was unusually free from maculæ.

	Greenwich M.T.	Dry Bulb.	Wet Bulb.	In Sunshine.
	h m	°	°	°
July 28	1 45	66	62·5	91
	3 15	61·5	59·5	65

Clouds affected the subsequent observations.

On the supposed Period of Revolution of the Great Comet of 1680. By Mr. Hind.

Of the many hundred comets recorded in history, that which has attained the highest celebrity is the famous body which became visible in Europe towards the end of the year 1680. Independently of its vast magnitude and brilliancy, its train of 90° and its nucleus "glowing like burning coals," it is remarkable as having attracted the attention of the great Newton to the subject of comets, the result of which was the discovery of the true theory of these bodies so zealously applied in practice by Halley. But there is one other point connected with the history of this comet which has excited great interest, I allude to the view taken by Halley, and subsequently by Newton himself, with regard to the time of revolution and the identity of the comet with several others of considerable magnitude mentioned in history.

In the *Synopsis of Cometary Astronomy*, published by Halley, we have a statement of the grounds upon which he considered himself justified in announcing the periodicity of the comet, which depend entirely on historical details. He remarked, if any argument could be drawn "from equality of periods and similar phenomena, that wonderful comet which appeared in the year 1680 was one and the same with that of 1106, when Henry I. was King of England: it first emerged out of the sun's rays on Friday, February 16, in the evening, and was seen for a long time afterwards every evening. The star which seemed little and obscure appeared in the south-east. But the ray which proceeded from it was very clear and large, shining towards the N.W., as we have it

recorded in the *Saxon Chronicle* by one who seems to have been an eye-witness." This description, says Halley, agrees very well with that of the comet of 1680, both in respect to the length of its tail and also its situation in regard to the sun. He then draws attention to another comet which was seen during the consulate of Lampadius and Orestes, in 531, in the reign of the Emperor Justinian, and is thus recorded by Malala, the author of the *Chronicle of Antioch*, "A great and fearful star appeared in the west, emitting a white beam upwards, which, as it appeared like the flashes of lightning, some called it *Lampadian;* it was seen for twenty days." Malala does not mention the time of the year this happened; and it is evident that Halley had no knowledge of other descriptions, particularly that of Theophanes, who tells us the comet was seen in September.

Again, it was remarked by Halley, that if we reckon backward another period of 575 years, we shall come to the 44th year before the Christian era, when, soon after the death of Julius Cæsar, a very remarkable comet appeared, mentioned by almost all the historians of those times, and by Pliny in the second book of his *Natural History*, where we have the words of Augustus Cæsar himself concerning it: "In the very days of my games, a hairy star was seen for seven days, in that part of the heavens which is under the *Septentriones;* it arose about the eleventh hour of the day, and was clearly to be seen all over the world." The games here alluded to were dedicated to Venus, and commenced on the 23d of September, the birth-day of Augustus Cæsar. Halley understands by "the Septentriones," the seven bright stars of *Ursa Major*, popularly termed "*Charles' Wain;*" and instead of the eleventh hour of the *day*, he substitutes the same hour of the night, or about four o'clock in the morning at Rome: then, he considers that the description of Augustus is fully represented by the elements of the grand comet of 1680, supposing it to have been in perihelion about the 18th of September.

Thus far Halley endeavoured to trace the history of the comet in past times, and so well convinced he appears to have been of the accuracy of his conclusions, that he gave, at the expense of a great amount of time and labour, a table to facilitate the calculations relative to a comet revolving round the sun in 575 years.

But some writers have attempted to trace the comet into more remote antiquity. Adopting the periodic time assumed by Halley, they consider it must have appeared about 618 years before the Christian era, and refer to that epoch the comet mentioned in the *Oracles of the Sybil*, and even imagine that the prophet Jeremiah, in his first vision about this time, may have beheld the same body, which was to be a sign of forthcoming events. Reckoning backward another period of 575 years, they are able to bring in the lost Pleiad, Electra, who, being unable to view the sight of the destruction of Troy (B.C. 1194), abandoned the company of her sisters and retired towards the north pole of the world, where she appeared a long time in tears and dishevelled hair. This fable our

authors think may have had its origin in the appearance of a great comet, which moved from the Pleiades to the north pole,—a track perfectly accordant, they say, with that of the comet of 1680, under certain conditions. And further, the words of Varro, in relation to the planet *Venus*, which is stated to have changed its colour, figure, and path, about the time of the Deluge of Ogyges, are referred to a comet, which is found to accommodate itself to that of 1680, as regards the year of appearance (about B.C. 1770), and great brilliancy. The fanciful notions of Whiston relative to this comet, its agency in causing the universal deluge and final destruction of the earth, are too well known to require mention here.

My object in the present remarks is to show that neither *history nor calculation* will support the presumed periodicity of the comet of 1680, and had Halley been in possession of the various accounts of the ancient comets which have been collected together since his time, it is tolerably certain that he would never have started his hypothesis. His conclusions were perfectly justifiable on such data as he possessed; and it is necessary to say thus much that my reasons for offering these observations may not be misunderstood.

The descriptions which are left us of the comet observed in the time of Augustus Cæsar and in the reign of Justinian are so vague that they may be satisfied by twenty different orbits, and hence no trustworthy conclusions can be obtained from them alone. The account of the comet of 1106 is much more definite, though still far from perfect; and it is to this year that we must have recourse, to test the Halleian period of the great comet of 1680. If the positions of 1106 are found irreconcileable with the elements of this body, the main foundation upon which the period rests will be destroyed; and as no other large comet is mentioned about that year, we shall be compelled to admit that history is hardly confirmatory of the revolution assigned by Halley and Newton. And our doubts on this point will be still further increased if there should exist any difficulty in reconciling the vague relations of 531 and B.C. 44 with the orbit of the comet of 1680.

Nearly a century ago, Mr. Dunthorne communicated to the Royal Society an extract from a Latin manuscript then preserved in the Library of Pembroke Hall College, Cambridge, relative to the comet of 1106, which is the first on Halley's list. It is there stated that the comet was seen at the beginning of the sign *Pisces*, the tail extending to the beginning of *Gemini*, on the last night of *June* (or, as Mr. Dunthorne reads, *Jumedi*), in the year of the Hegira 499, on a *Wednesday*. It followed the order of the signs until it arrived at the commencement of *Cancer*. In justification of the correction which Mr. Dunthorne has applied to the date, he remarks that the last day of *June* fell on *Saturday* in 1106; but the last day of the Arabic month *Jumedi*, J. occurred on a Wednesday, the day stated in the manuscript. Supposing the comet of 1680 to have reached its perihelion on February 4, "for it must have been seen two or three days after it had passed that point,"

Mr. Dunthorne computed some places of the comet, and thought the comparison of them with the manuscript account must lessen, if not quite overbalance, the probability of the identity of this comet with that of 1680.

It is one characteristic of the elements of the latter body that *in less than five minutes* after its perihelion passage the latitude becomes *north*, and must remain so until about 25 days before another perihelion is passed; consequently, the latitude is a most important test of the identity of this comet with any other recorded in history; and it is from a contradiction in this respect that I think the strongest evidence against Halley's supposition is adduced. It will tend to facilitate a clear understanding of the case if the principal circumstances related of the comet of 1106 are collected here.

On the 4th of February in that year, or, according to others, on the 5th, a star or comet was visible in full daylight, from the third until the tenth hour of the day, and was distant from the sun only "one foot and a half," as historians express it. Matthew Paris and Matthew of Westminster are particular in terming this object a *comet*, though it is called a *star* in the Belgian Chronicle, and by several French authorities.

On the 7th of the same month a comet was discovered in Palestine at the commencement of the sign *Pisces;* it was seen in that part of the heavens where the sun sets in winter (*i.e.* about the W.S.W. point of the compass), the tail extended to the beginning of *Gemini*, *below* the constellation *Orion*. The latitude at this time was, therefore, undoubtedly *south*, or the train could not have had this direction. The fact itself is mentioned by two historians. On the 10th, the Chinese saw the comet at the *end* of the sign *Pisces* with a tail 60° long. About the 16th or 18th of February it was more generally observed. It had passed from the southern to the western part of the heavens, and was full west soon after sunset. After some time, it appeared between the west and north with a tail extending towards the north-east, and was visible until midnight. For *twenty-five* days it shone in the same manner at the same hour, and seemed to have no other movement but that of the sun, which gives it a real motion from west to east. The Chinese saw it traverse the constellations, or rather sidereal divisions *Kouei* (commencing at ζ *Andromedæ*), *Leou* (at α *Arietis*), *Mao* (at η *Tauri*), and *Pi* (at ε of the same constellation); in other words, it passed from the end of *Pisces* to the end of *Taurus* before it was lost to view. Historians differ as to the time it remained visible, some assigning a duration of little more than a fortnight, while others, and by far the greater number, extend the period of observation to 40, 46, or 50 days, until it was seen with difficulty (with the naked eye).

Now, to bring the comet to the *end* of the sign *Pisces*, on February 10, as it was observed in China, we must fix the perihelion passage with the elements of 1680 on the 4th of the same month; but here we meet with a difficulty which in my idea is

sufficiently conclusive against the identity of the comets of 1680 and 1106. On the 7th, when it was observed in Palestine, the tail was observed to extend *below Orion*, the head appeared in the W.S.W., and the latitude, as already remarked, must have been *south*, but the comet of 1680 *cannot* have a south latitude at this interval after the perihelion passage. Again, we meet with another objection in a circumstance mentioned by an anonymous author quoted by Struyck, who says the comet advanced to the beginning of *Cancer*, which will not agree with the path calculated from the elements of 1680. These facts argue strongly against the identity of the comets, especially the difference of latitude,—an insurmountable objection. To reject the testimony of ancient historians for the sake of maintaining a preconceived notion on this point, would surely be unjustifiable, particularly when all the observations we possess may be faithfully represented by an orbit, though widely different in its elements from those of the comet of 1680. After the experience we have had in the case of the great comet in March 1843, it cannot be doubted that the "star" seen so close to the sun on the 4th of February, 1106, was really the comet which became conspicuous in Palestine on the 7th of the same month. Pingré, it is true, expresses doubts on this point, but at the time he wrote the visibility of the comet of 1106, only a few degrees from the sun's limb, was without a parallel in the history of cometary astronomy. The grand comet of 1843 was discovered in full daylight and less than three degrees distant from the sun, on the day of perihelion, in Italy, in America, at the Cape of Good Hope, and other stations.

Let us now examine the few particulars which have reached us us respecting the comet mentioned by the historian Malala, A.D. 530 or 531, which Halley considered also identical with that of 1680, I say 530 or 531, for the fact is, there exists some doubt as to the year the comet was observed. Cedrenus places it in the fourth year of Justinian which appears to be 530, while Zonaras in his *Annals* defers it until the fifth year of Justinian, therefore till 531. The comet mentioned by the European historians was visible in the evenings in September in the western heavens, the tail extending upwards towards the zenith, in which case it could not have been that of 1680, which can only be observed in the mornings during the autumn. Father de Mailla tells us that the Chinese observed a comet in the 9th moon of the 47th year, 54th cycle, or in October 530, adding, that it was observed from *Arcturus* to λ and μ *Ursæ Majoris* (from *Takio* to *Tchong-tai*), a remark which may either refer to the direction of motion of the head or to the direction of the tail. This last I consider most probable, as we meet with other instances of a similar kind in the Chinese accounts of comets. In a paper upon the past history of Halley's comet, presented to this Society in January 1850, I have shown that the Chinese comet of October 530 might, very probably, have been Halley's, if De Mailla's remark refer to the tail; this author was no great astronomer, and his descriptions are frequently much confused from want of a proper

understanding of the difference between the Chinese asterisms and sidereal divisions. Pingré, in his *Cometography*, distinguishes the European comet of 531 from the Chinese comet of 530, adding that the relation of De Mailla will not agree with the account furnished by Theophanes and other Europeans, though it accords well with the theory of the comet of 1680, supposing it to refer to the movement of the head; and hence he thinks the Chinese and European comets were different bodies, and that the former may have been identical with the great comet of 1680. I confess I do not see this in the same light. It appears more natural to conclude that the same body was observed in Europe and Asia, an error of a year being admitted in one relation, probably in the Chinese; and I think Pingré's objection to this inference may be overcome by allowing the various particulars to refer to different dates, the Chinese observing the comet when it was, perhaps, not very favourably situated for Europeans. It is useless, however, to reason upon such vague accounts, and I shall merely add that orbits entirely different from each other will agree equally well with the few particulars that have descended to us respecting the comet of Malala.

The comet of Augustus Cæsar is liable to equal uncertainty. In the same year, but in June instead of September, the Chinese observed a comet in the north-west, with the same right ascension as the constellation *Orion;* and it is certain if this be the object mentioned by Augustus Cæsar, it could not have been the comet of 1680. But there is a probability in favour of the appearance of two comets in B.C. 43, the first being that of the Chinese historians, and the second the comet of Augustus and the "unknown star" of Dion Cassius, which shone for a long period. Now, the only one which affords the slightest indication of identity with the comet of 1680 is that of Augustus, and in order to make this identity plausible, M. Pingré has suggested several alterations or different interpretations of the original for which it is not easy to admit the necessity. His discussion will be found in the second volume of the *Cometography*, p. 138, but after all this able astronomer remarks that the circumstances, thus revised or altered, would accord as well with the elements of a great number of other comets, as well as with those of the comet of 1680. Even supposing, therefore, that it is possible to reconcile the description of Augustus Cæsar, under the original interpretation, with the orbit of that comet, it affords but very doubtful evidence in favour of identity, since a dozen other comets might have been viewed under the same circumstances. Dion Cassius particularly mentions that the "unknown star" was visible a long time, and on this account it seems not unlikely that it may have been the same as Cæsar's comet, and the comet of the Chinese; in which case its identity with the comet of 1680 is out of the question.

Thus far then we have the testimony of history. But as I have already remarked, calculation is against the truth of the Halleian period of the comet, that is, the discussion of the observations taken in the years 1680 and 1681 leads to results which cannot

be reconciled with it. In the *Zeitschrift für Astronomie* for 1818 is a remarkable memoir upon the comet of 1680, published without name, under the motto "*Et voluisse sat est*," but due to Professor Encke, then of the Seeberg Observatory, and now the well-known astronomer of Berlin. It is impossible to speak in too high terms of the execution of this elaborate paper, which contains an accurate reduction of all the observations, and a rigorous computation of the most probable elements of the comet's orbit, in which the author has availed himself of all the appliances of modern science. Before stating the conclusions arrived at in this memoir, it will be desirable to give a brief recapitulation of the circumstances connected with the apparition of the wonderful comet of 1680.

Godfrey Kirch, of Coburg, in Saxony, was the first discoverer of this body, which he met with on the morning of the 13th of November, as he was about to observe the moon and planet *Mars*. There were several small stars in the field of the telescope with the comet, and he determined its position by estimation with respect to them. The observation is a very fair one, the stars of reference were all situate within a space of little more than a degree, and their places are found in the *Histoire Céleste* of Lalande. Encke considers that the resulting position of the comet is uncertain to the amount of 90 seconds of arc only; and any one who will take the trouble to lay down the places of the comet and stars upon paper, and then read over the observations of Kirch as detailed in the *Philosophical Transactions*, vol. xxix., must, I think, readily admit that this is as large an error as a careful observer would be likely to make. It is necessary to be satisfied upon this point, because the first observation by Kirch has an all-important bearing upon the determination of the true form of the comet's orbit. Observations were taken by the same astronomer on the mornings of the 16th and 21st of November, but they are too rough to be of service. The same remark applies to nearly all the observations taken elsewhere before the perihelion, including those of Ponthäus, Cellius, Gallet, Montenarus, &c. The comet was detected by Flamsteed, at Greenwich, on the 22d of December, and was observed by him fourteen times between that date and the 15th of February. Cassini observed it on eight evenings during the three weeks following the 29th of December. The latest positions were obtained with great care by the illustrious Newton between the 7th and 19th of March. In addition to the observations of Kirch, Flamsteed, Cassini, and Newton, which possess a considerable degree of accuracy, a great number of others were taken in 1681, in various parts of Europe and America; but with few exceptions they are extremely rough and of no service in the investigation of the elements.

In the memoir to which I have alluded, Professor Encke first deduces an approximate set of elements from five observations reduced by Halley, including the first by Kirch, three by Flamsteed, and the latest by Newton on March 19. He then rigorously reduces the whole series of positions by Flamsteed, Newton, and

Cassini, and determines the effect of planetary perturbations upon the geocentric places of the comet, having regard to the influence of *Mercury*, *Venus*, the *Earth*, *Mars*, *Jupiter*, *Saturn*, and *Uranus*. It is found that at no time during the visibility of the comet was its apparent position affected by perturbation to the amount of one second of arc. The next step is to deduce equations of condition for small variations of each element for the whole series of observations, including the early one by Kirch, and these equations are finally solved by the method of least squares. The result shows that the most probable period of revolution is 8814 Julian years, but that the observations may be represented within their probable errors by elements in which the extremes for the time of revolution are 6179 and 14,031 years, that is, it cannot be less than the former nor more than the latter.

The next step in the investigation is to ascertain how an ellipse with the Halleian period of 575 years would increase the errors; and for this purpose the equations are again solved, and the following important conclusions are obtained. The sum of the squares of the errors is four times as great as in the former solution, and the first observation by Kirch, which here becomes the *experimentum crucis*, cannot be represented within 10 minutes of arc, though $1'\frac{1}{2}$ is a fair allowance for its possible uncertainty; an error of $10'$ is out of the question. Here then we have the results of calculation decidedly opposed to the Halleian period of revolution.

Professor Encke next finds the most probable parabola, which agrees very well with the observations, and after computing the elements on the hypothesis of hyperbolic motion, though without a satisfactory result, the errors being as large as with Halley's period, he proceeds to determine the most likely orbit and its probable limits, from the observations of Flamsteed and Newton only; he finds that the time of revolution which best satisfies the whole is 3164 years, but that these positions may be represented within their probable errors by orbits in which the period may be any length of time greater than 805 years. But when we come to compare the elements with Kirch's first observation, the agreement is not so good as in the former case, where all the equations were solved together. Professor Encke arrives at the conclusion that it is impossible to assign by calculation the exact period of revolution of this famous comet; but, he remarks, there is the highest probability against its being less than 2000 years. The most legitimate assumption which the researches of this great astronomer enable us to make, is that the revolution of the comet is accomplished in a period somewhere between 6000 and 14,000 years.

It is worthy of remark that in the case of a comet with a revolution of many centuries, it is very improbable that several successive periods between the perihelion passages should be equal within one or two years. It is almost certain that in an orbit so excentric as that of the comet of 1680, planetary perturbations would greatly alter the successive periods; for, as Encke shows, notwithstanding the small effect of these disturbances upon the geocentric

place, their influence upon the major axis is very considerable, owing to the enormous excentricity and the smallness of the parameter. Even in the three months following the perihelion passage of the comet of 1680, its period, supposing it to have been 575 years at this epoch, would have increased by nearly five years, and in a whole revolution the difference might be vastly greater.

I think any person who impartially reviews the evidence of history and calculation, which is here collected together, must be led to the conclusion that the Halleian period of the great comet of 1680 cannot be admitted. I have been the more anxious to submit these observations to the Society, inasmuch as, s far as I am aware, the particulars are not at present to be found in any English work, and it seldom happens that an elementary treatise on astronomy issues from the press without a repetition of the old story respecting this comet, which is frequently given in the most positive terms, and arguments founded upon it which can have no pretensions to accuracy or even plausibility. It is no compliment to either Halley or Newton to suppose that if the historical and other evidence now in our possession had been before them, they would have started the hypothesis of a periodic revolution of the comet of 1680 in 575 years.

On a Method of converting the Motion of a Pendulum-clock into an equable Motion of Rotation. By Mr. R. L. Jones, F.R.A.S.

I have for some time been endeavouring to make a cheap and accurate clock for moving an equatoreal telescope, and I think I have perfectly succeeded.

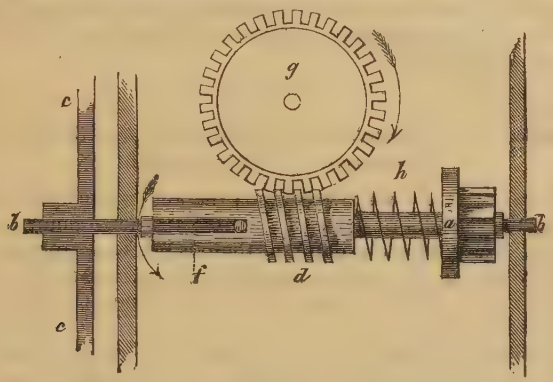
The principle proposed was that of governing the time by an ordinary pendulum, and equalising the motion by a fly-wheel. A spring has been used for this purpose by others, but I am not aware that it has been applied in a manner similar to mine.

The power is obtained by a weight suspended by a catgut cord, a little thicker than that of a common clock, and which is wound on a barrel $3\frac{1}{2}$ inches diameter. Attached to this barrel is a toothed wheel, with which another wheel works that is keyed on the axis of the tangent-screw; this carries the telescope in the ordinary way.

The barrel turns four times in the hour, and requires winding once in three hours, but might be made to go considerably longer without difficulty. From the wheel attached to this barrel there is a series of wheels and pinions to gain speed, and the last pinion revolves once in a second; the fly-wheel, which is very light, but 12 inches diameter, is keyed to the axis of this pinion. On the same axis is a tangent-screw, which, by means of a slot, and a pin through the axis, is free to slide along the axis, but must turn with it. The tangent-screw turns a wheel with thirty teeth, giving it one revolution in 30 seconds. On the same axis with the last-named

wheel, is an escape-wheel of thirty teeth, which works the escapement, and a pendulum of the length to beat half-seconds.

The parts last described require a sketch.



(Full size.)

a The last pinion of the series.
b Its axis.
c A portion of the fly-wheel.
d The tangent-screw.
f The slot and pin.
g The wheel on the same axis as the escape-wheel.
h A spiral spring of thin brass wire.

When the motion of the wheel *g* is suspended by the escapement, the continued motion of the fly-wheel and axis forces the tangent-screw to the right, compressing the spring. When the wheel is unlocked, the spring returns the tangent-screw to its former place and moves the wheel *g* one tooth.

It is evident the only irregularity in the motion of the fly-wheel arises from its having once in each revolution to compress the spring; but for the purpose of keeping the pendulum in motion the spring is required to exert a very small recoil force, and it is compressed to an extent equal only to the breadth of one tooth of the wheel *g*: the effect of this cannot be noticed.

Any tendency in the fly-wheel to increase its speed beyond what is needful to move one tooth of the wheel (*g*) in each revolution, is controlled by its increased pressure on the spiral spring.

The weight required to work the clock and the telescope is 16 pounds; the telescope is 6 feet focus with a clear aperture of 4 inches. From a number of experiments I should say the power is divided as follows:—

	lbs.
To carry the telescope	1
Pendulum	2
Fly-wheel	11
Friction	2

When the telescope is tolerably poised, the power to drive it is

so small a portion of the whole, that a little inequality in the poise or a moderate wind has no sensible disturbing effect. The clock will gain or lose a little as the power alters the arc of vibration of the pendulum; but this will not be much, and may, perhaps, be sufficiently compensated by adopting a scapement between the recoil and dead beat.*

Observations of the Satellites of Uranus, made with the 20-feet Equatoreal. By Mr. Lassell.

"In the first column of positions, the angles are given with reference to the parallel of declination, from the north point round by east; and, in the adjacent column, the positions are approximately reduced to the angles on the apparent orbits, reckoned from the ascending node round by north in the direction of the motions of the satellites.

"The proportion of the transverse axis to the conjugate, as the orbits are at present projected, is nearly as 8 to 6, and the inclination to the ecliptic is about $82°\frac{1}{2}$ s. p. and n. f.

"In every instance the positions and distances of (1) and (2) are the results of *estimations*,—generally from the *measured* positions and distances of the two bright satellites. And the first two satellites discovered by Sir Wm. Herschel in 1787, usually termed "the 2d and 4th," or "the bright satellites," are here designated by the Roman numerals III and IV.

	(1.)			(2.)			III.		IV.	
	Positions.			Positions.			Positions.		Positions.	
Green. M.T.	Paral^{l}.	Orbit.	Dist.	Paral^{l}.	Orbit.	Dist.	Paral^{l}.	Dist.	Paral^{l}.	Dist.
1851.	°	°	″	°	°	″	° ′	″	° ′	″
Oct. 24·4375	163	275	...	354	77	...	168 26	30·83	156 11	39·92
28·5	325	115	...	350	84	...	358 0	29·92	33 0	...
30·4791	35	30	10	169	268	...	268 9	23·28	350 23	40·99
Nov. 2·5	320	121	12	273	163	13·8	160 43	28·91	272 47	29·39
12·4375	332	106	...	131	311	...	112 34	...	5 0	...
17·4375	337	101	...	47	19	...	257 34	24·09	220 12	34·63
18·5	186	242	...	316	126	...	202 0	...	191 6	36·83
21·4889	123	318	...	63	7	...	91 7	21·38	131 57	34·29
22·4833	332	106	...	329	111	...	38 50	26·03	101 32	...
27·4798	345	91		not seen			194 25	18·61	325 31	38·12
Dec. 11·3750	150	291		not seen			340 0	...	304 0	...
16·4132	160	280	13·5	70	0	14	147 0	...	172 19	...
22·3541	...	...	...	...	...	...	248 26	...	10 2	39·46

Observations and Remarks.

Oct. 24. Sky pretty favourable; the new satellites (1) and (2) seen certainly for the first time.

---

* In extreme cases, the pendulum might be suspended from a string between cycloidal cheeks, as in Huyghens' clocks.

Oct. 28. The sky had only recently cleared at $11\frac{1}{2}$ hours, after a rainy and stormy evening; and though the new satellites were certainly seen, the opportunities afforded between the passing clouds only sufficed for a careful diagram of situations, and a single measure in distance of Satellite III.

Oct. 30. An exceedingly faint star was noticed, distant about $3\frac{1}{2}'$ and position about 150°, but no opportunity occurred of ascertaining whether it was or was not a satellite The measures of the old satellites good and satisfactory.

Nov. 2. Sky stormy and many clouds passing, with a disturbed, though transparent, atmosphere. Exactly at midnight (2) was in the line joining the centre of *Uranus* and IV, and at rather less than half the distance of IV from the planet. At a later period of the night it had evidently passed that line of conjunction.

Nov. 12. Sky hazy, and the faint satellites seen only with great and painful attention. No *measures*, except a single one of III in position, could be got.

Nov. 17. Very keen frosty wind; air indifferent, yet the measures, laboriously taken, are, I think, good. No other stars around *Uranus* at all likely to be satellites. I surveyed the planet some time, with a wheel of, I believe, very excellent concaves, made for me by Mr. Andrew Ross; principally using powers 540 and 351, as they seemed most efficient: but the faint satellites scarcely could be recognised with them, and certainly they were much less evident than with the positive Munich eyepiece 614, or the (Ross) Huygenian 778. I cannot, therefore, corroborate the impression that concaves are more luminous than other eyepieces, in a trial which I should almost think an *experimentum crucis*. I tried the various powers of the wheel up to 2300; and on other occasions I have made desultory comparisons with concaves, leading to a similar result. They perform best when the powers are low.

Nov. 22. At $9^h\ 30^m$, (2), generally the faintest, is nearly in conjunction with (1), or rather a little in advance of the right line joining (1), and the centre of the planet. Atmosphere variable and frequently hazy; but the measures obtained are worthy of confidence. On this occasion the full aperture of the telescope was more efficient than any reduced aperture in facilitating the measures of both positions and distances.

Nov. 27. Measures pretty good, though I was unable to obtain a certain view of (2), the faintest. On calculating afterwards I found that it would be then at about its nearest approach to the planet.

Dec. 11. The air so exceedingly unfavourable that I could only make a diagram of the situations of three of the satellites, (2) being again invisible.

Dec. 16. All the satellites well seen for a short time, but clouds coming on prevented more than a careful diagram being made, and a single measure of IV being obtained; III and IV equal in brightness, (1) very decidedly fainter, yet brighter manifestly than (2).

Dec. 22. Vision very indifferent, yet the measures, carefully taken, are, I believe, good.

"The most probable periods of revolution I have been able to deduce for the new satellites from this series of observations are, $2^d\ 12^h\ 18^m\ 58^s$ for (1), and $4^d\ 3^h\ 33^m\ 26^s$ for (2). With these data I have been led to look back to the observations I made of suspected interior satellites in 1847, and published in the *Monthly Notices*, vol. viii. p. 44, but, by a typographical error, headed 1848 instead of 1847. On reference to the original observations and diagrams, from which the angular positions were deduced, I find that a position-angle of 350° on the 14th September better suits the diagram than 10°. Also, that the observation of 29th September is very questionable, there being only a rude diagram made, without any confirmatory remark appended: I therefore

reject it. But the night of the 6th November was remarkably fine, and a very careful diagram was made of the four satellites in respect to position and magnitude; and remarks are appended which sufficiently attest the correctness and certainty of the observations. I find, however, that the diagram has not been correctly translated into angular positions; and, rejecting altogether the observations of the 29th September, a table of the series will stand thus,—

		(1.) Positions.		(2.) Positions.		III. Positions.		IV. Positions.	
		Parall.	Orbit.	Parall.	Orbit.	Parall.	Orbit.	Parall.	Orbit.
	1847.	°	°	°	°	°	°	°	°
Sept.	14·567	350	84						
	27·408	326	114						
Oct.	1·521			348	87				
Nov.	6·429	346	89	126	315	292	148	344	92

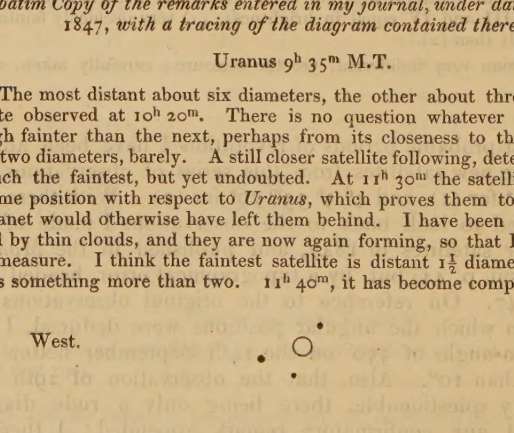
"Presuming that on the 6th November, 1847, I obtained undoubted observations of the four satellites,* and that (1) was observed on the 14th and 27th September, and (2) on the 1st October, the respective periods given by comparisons of these observations agree with the recently deduced periods within very small limits of error. And comparing the observation of 6th November, 1847, with that of the 2d November, 1851, interval = 1457·071 days, and concluding that 580·114 revolutions of (1) took place in that interval, I obtain for its period 2·5116998 days, or $2^d\ 12^h\ 16^m\ 50^s{\cdot}86$. In like manner, concluding that 351·5694 revolutions of (2) were performed in the same time, its period comes out 4·144477 days, or $4^d\ 3^h\ 28^m\ 2^s{\cdot}91$.

"In October 1847 I estimated the proportions of the axes of the orbits to be as 10 to 5·15, and that the major axis was inclined to the ecliptic $87°\frac{1}{2}$."

* *Verbatim Copy of the remarks entered in my journal, under date 6th November, 1847, with a tracing of the diagram contained therein.*

Uranus $9^h\ 35^m$ M.T.

"The most distant about six diameters, the other about three. The closest satellite observed at $10^h\ 20^m$. There is no question whatever of its visibility. Though fainter than the next, perhaps from its closeness to the planet, distant about two diameters, barely. A still closer satellite following, detected at $10^h\ 50^m$: by much the faintest, but yet undoubted. At $11^h\ 30^m$ the satellites retain nearly the same position with respect to *Uranus*, which proves them to be satellites, as the planet would otherwise have left them behind. I have been repeatedly interrupted by thin clouds, and they are now again forming, so that I fear I shall not get a measure. I think the faintest satellite is distant $1\frac{1}{2}$ diameter, and that the next is something more than two. $11^h\ 40^m$, it has become completely cloudy.



Observations of Neptune and his Satellite. By Mr. Lassell.

1851.	G.M.T. h m	Position. ° ′	G.M.T. h m	Distance. ″
Aug. 29	12 0	218 35	12 0	15·14
Sept. 15	11 30	210 9		
Oct. 16	10 15	232 26	10 15	11·25
24	8 22	41 13	8 2	15·60
Nov. 17	7 30	44 52	7 30	13·58
22	6 21	34 26	6 38	13·27

Observations and Remarks.

Aug. 26. Sky very variable, sometimes brilliant, and sometimes almost entirely overcast. Wind violent from about west. Turned to the planet about 11^h. I thought the view remarkably good for the circumstances. Power 219, with which the satellite was immediately seen, and an evident appendage, such as I used to take for a ring, but more evident on the south side, nearly at right angles with the parallel of motion. The same appearance remained with 366, but scarcely so strikingly, and it vanished with 614. At this time I was observing with the new speculum (C), mounted with the system of levers I have elsewhere described (see *Monthly Notices*, vol. xi. p. 165), which totally removes the indications of flexure formerly discernible. I had also, with power 614, a strong suspicion of a second satellite, but though I saw a point there positively for a short time, I could not afterwards see it. The sky, however, was very variable, and clouds and haze coming on prevented my obtaining any measures, even of the known satellite. Estimated position of satellite 40°; position of suspected satellite, 145°; rude estimation of distance of suspected satellite, 8″. The position of the axis of the suspected ring is in the direction of 5° and 185°.

Aug. 29. I received again an impression of a ring-like appendage, but it is principally on the south side, and is nearly at right angles to a parallel of declination. The measures taken to-night are worthy of confidence.

Oct. 16. Sky very unfavourable, and frequently obscured by passing clouds. Measures obtained with great difficulty.

Oct. 24. Measures remarkably good and satisfactory.

Nov. 17. Air pretty good, and measures very satisfactory.

Nov. 22. Wind north, and sky brilliant, but the planet very ill defined, and measures were taken with great difficulty. Air decidedly bad. Yet, on turning the telescope at a later hour upon *Uranus*, I saw the faint satellites almost immediately; and I received quite a different impression of the quality of the air. The improvement is probably chiefly owing to the difference of altitude; for I scarcely ever remember seeing an object with the 20-foot telescope and a power of 600, (which is necessary for coping with *Neptune's* satellite,) without being, even at the present meridian altitude of that planet, greatly annoyed by atmospherical disturbances.

Divisions of the Ring of Saturn. Extract of a note from Mr. W. C. Bond.

On October 20th, 1851, the broad inner ring of *Saturn* was seen to be divided into a succession of narrow rings for two-thirds of its breadth, the dark stripes being most conspicuous on the side of the ring nearest the ball. Much in the manner described by Mr. Dawes (*Monthly Notices* for November 1851). The state of the atmosphere was unusually favourable, so that, even with a power of 800, the minute subdivisions were quite apparent. Attention was not directed to the outer narrow ring; but with a power of 140 no new division was noticed.

Note on the Dark Ring of Saturn. By Mr. Dawes.*

In the year 1838, Dr. Galle, with the large refractor at the Berlin Observatory, discovered the luminous portion of the faint ring, and on four different nights measured its breadth from the inner edge of the bright ring. But as the phenomenon was not seen by any other observer, or its real nature completely made out, the discovery attracted little or no attention on the Continent, and seems to have been entirely unknown both in this country and in America. Subsequently, the great southern declination of the planet precluded advantageous observations of it in these latitudes; and thus Galle's discovery remained unconfirmed for twelve years.

The astronomical instruments of the late Mr. Children are to be disposed of; they are in good condition, and may be seen at the apartments of the Royal Astronomical Society.

A very fine equatoreal with clock-work, the telescope about 66 inches focal length, aperture 4 inches: the object-glass by Mertz, and the mounting by Troughton and Simms.

A 30-inch transit and iron stand by Troughton and Simms.

An 8-inch altitude and azimuth by Troughton and Simms.

Two level collimators by Troughton and Simms.

A large microscope by Dollond, and a diploeidoscope by Dent.

ERRATA

In the early copies of the January Notice.

Page 43, line 19, *for* Colonel Silverstopfe, *read* Silverstolpe.
— 45, last line, *for* 70°, *read* 45°.
— 46, line 1, *for* 100°, *read* 80°.
— — — 2, *for* 145°, *read* 110°.
— 47, — 34, *for* RINGERIGEL, *read* RINGERIDET.
— 49, — 29, 31, *for* JUNE, *read* TUNE.
— 64, — 42, *for* $3^h\ 56^m\ 54^s{\cdot}9$, *read* $3^h\ 55^m\ 54^s{\cdot}9$.
— 66, — 18, *for* RAVELSBERG, *read* RÆVELSBERG.
— 67, 2d line from bottom, *for* 22 inches, *read* 20 inches.
— 72, line 30, *for* Colonel Silverstopfe, *read* Silverstolpe.

In plate, *for* 11, *read* 14.

Monthly Notices, vol. x. p. 173, B.A.C. 3233 is set down as having a difference in R.A. $= -6^s{\cdot}71$. On a subsequent revision, it appears that B.A.C. 3233 *does not exist*, and that the star taken for it in Lord Wrottesley's list is B.A.C. 3226, which has nearly the same declination, and exceeds it 1^m in R.A.

Vol. xii. p. 61, Observations of MM. Swan and Lane, *read* Lat. 57° 42′ 58″ N., Long. $0^h\ 47^m\ 45^s{\cdot}3$ E.
— — — 62, line 25, *for* group of spots 1′, *read* $1'\frac{1}{2}$.
— — — —, — 27, 28, 29, *for* $3^h\ 8^m$, *read* $3^h\ 58^m$.

---

* This note should have been inserted in the last Annual Report, p. 103.

London:—Printed by G. BARCLAY, Castle St. Leicester Sq.